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Dear Phil:

I hope your airconditioning system is working this summer— at least if our weather has anything to do with yours!

As you know, I haven't been doing very much with Salmonella during the last year, except for some micromanipulation work. Do you remember those "trails" into soft agar that you often get with, e.g., TM2 —x various non-motiles? Anyhow, Bruce and I had figured that these meant cells which had received an abortive fragment which conferred motility but did not reproduce along with the cells. We have each been able to confirm this by direct microscopic experiments, motile cells having been picked early out of mixtures of O forms and phage. These give rise to progeny which are motile, but consistently divide to give one motile, one non-motile daughter for many generations (up to 60 in one experiment). The interesting thing is that the initial transducing phage particle carries not one fragment, but a large bundle of them that are sorted out during the first 5 or 10 divisions, so that the initial motile cell gives a family of anywhere from 5 to 100 motiles before these then settle down to the one motile/one-non-motile just described. There is an interesting genetic problem just what these "fragments" are— I think probably not genes after all (in this case) but non-reproducing gene products. But most of my time lately (between trips and hot spells) has been spent on looking for the sexual acts in E. coli K-12, and not long ago I had my first encouragement. The trick is to use one motile, one non-motile strain for the parents. With the "high frequency of recombination" strains one can then find a fair number of curious pairs, tumbling around each other, and joined laterally (by a so-far invisible connection). These separate after and hour or two, and the "exconjugants" then show a very high incidence of recombination, about 50-60%, so this is clearly the sexual pairing. Probably a nucleus is passed from one cell to the other; there are several nuclei in each cell, os this does not destroy either of them. The morphology still has to be worked out.

But back to Salmonella. In addition to the reprints from Iseki, I have been in touch with another Japanese (Uetake) who claims similar results, the transformation of E_1 to E_2 (or more often to a 3,10,15 type) by phage, and Aleck Bernstein here would like to confirm the results. In previous requests for strains, I had emphasized groups B and D, and we have very few group E strains. I attach a list with my request for your assistance. Have you been engaged in any of this at all?

Aleck also has done a few transductions with S. wien and S. dar-es-salaam to try and clear up the genetics of the lw factor. Since the transductions so far have only gone one way, the results are not so certain, but it looks (as one might expect) as if lw in S. dar-es-salaam is a phase-1 homolog; lw in wien is less cer-

tain: it probably is phase 2, but still might be an aberrant (duplicated) phase 1 like N97 java. Anyhow, if I may, I am sending a few strains under separate cover (as described on enclosure) for serological confirmation.

I am sure to have missed Pittsburgh and a change to talk things over with you. Aleck has a rather confusing story on the ~~spp~~ fertility of the specific coliforms, and we may be begging some more strains of E. coli 055 and 0111 if you have been handling any more outbreaks.

Has anything else come in of interest-- any O forms, for example? I know how overwhelmed you are with the routine work, but I am always looking forward to another review, for example, of the types that come in.

One point in this connection does trouble me. As I understand it, the State of Wisconsin (and others) makes a legal distinction between typhi, paraB as against other types like typhimurium. In view of the increasing confusion of bacteriological types in different clinical pictures (cf. your Ky Bull 525, p.34, second paragraph, last sentence) shouldn't these matters be reviewed? At any rate, I wonder if you could help me find any compilation of the current practices of the different states insofar as different serotypes are dignified with different legal restrictions (in certificates for food handlers etc.)

Another question came up in a conversation with Dubos (on the TAB vaccine experiments). We were both surprised to learn of the failure of prophylactic vaccination for pullorum-gallinarum. If this is right, it should be rather disturbing for the TAB problem, shouldn't it? Or is there some special angle on the bird infections. Anyhow, Dubos would like to know if there is <u>any</u> animal Salmonellosis (especially if in a convenient experimental animal) where one can demonstrate protective prophylactic immunity against the "natural" disease.

Please give best regards to your associates and assistants from Esther and myself. Any chance at all of ever seeing you in these parts?

Yours,

Joshua Lederberg